sylvestre should have put the editor on his guard; the reference to Fuchs which he gives shows clearly that some *Allium* was intended, and tracing the synonymy through Bauhin to Linnæus, we find that *A. vineale* is the species meant. Even apart from this evidence, it is obvious that the "sellers of simples" who substituted another herb for *Teucrium Scordium* would have selected one that had a similar smell, and not one like the *Colchicum*, which has no such odour. Mr. Mowat rightly identifies the "gosegresse" of the "Sinonoma" (p. 41) with *Potentilla Anserina;* but it is hardly accurate to say that it is "generally cleavers" (*Galium Aparine*). The "Dictionary of English Plant-Names" shows that the *Potentilla* is at least as frequently called goosegrass nowadays; and it is the "Gosgres" of the Old English Medical MS. printed in Archæologia, vol. xxx. (p. 408)—a glossary containing many plant-names which have been too little noticed. "Caputpurgium, *i.* stafisagria," is not *Pedicularis*, as glossed by Mr. Mowat—a plant supposed to favour the growth of lice—but the Staves-acre, which has been used for destroying them since the days of Pliny. Similarly "Calendula, *i.* solsequium," is not Caltha, but the Marigold, *Calendula officinalis;* this is made quite clear by the description under Kalendula, which may be cited as an example of the capital diagnoses which the glossary contains. "Kalendula est herba crescens in hortis portans florem rubeum vel croceum de quibus floribus faciunt sibi juvenculæ coronas, solsequium idem."

To make a glossary of this kind useful to other than English-speaking students, the plants should be identified with their Latin as well as by their English names. Even in America, the mention of the cowslip would suggest, not *Primula veris* but *Caltha palustris*. Mr. Mowat says (quite correctly) that "pigle, pagle, paigle seems to be the regular old name for cowslip"; and he seems to imply that the entry "pigle, *i.* stichewort," may also refer to *Primula veris*. But a previous entry, "Lingua avis, *i.* stichewort, *i.* pigle," is quite sufficient to confirm the natural conclusion that by stitchwort *Stellaria Holostea* (which is still commonly so called) was intended; and this plant is called pigle by Gerard in his appendix of "names gathered out of ancient written and printed copies, and from the mouthes of plaine and simple countrie people." Under "Serpillum" we find the name "pelestre," which Mr. Mowat queries "palustre?" but this is a form of Pellitory, already given on p. 34—"Piretrum, pelestre idem"—the name "Piretrum" showing that *Anacyclus Pyrethrum*, not *Parietaria*, was intended; the Anacyclus also figures in the Glossary under the name of "Dentaria," in allusion to its former use in toothache. An instance of the insufficiency of English names is given in the gloss of "Umbilicus veneris, *i.* penigresse," as "penny-grass, pennywort"; it is of course *Cotyledon*, not *Hydrocotyle*, which is here meant, though the vernacular names are common to the two plants.

In most cases, however, where Mr. Mowat has given a modern synonym, it is correct; but I do not quite understand why only comparatively few plants are identified, as the identifications are by no means confined to cases of special difficulty. Some very obvious explanations are duly set forward, while in more doubtful cases help is often not forthcoming.

In the volume of Plant-Names which I hope to prepare for the Early English Text Society, I shall try to identify as far as possible all the plant-names, both English and Latin, with their modern scientific equivalents. This will be a tedious work, and one in which mistakes are certain to occur; but a foundation will then have been laid for the future production of a comprehensive work on English plant-names which shall take in all, from the earliest to the most recent. When such a work comes to be done, the great value of collections like this of Mr. Mowat will become apparent.

JAMES BRITTEN

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Analysis of the Tuning Fork

MR. HERMANN SMITH, in a letter in NATURE last week, commenting upon my paper read before the Physical Society on June 10, of which you gave a short report, offers some very cogent experiments in support of the evidence I have endeavoured to give, that the tuning-fork does not communicate its sonorous vibrations to a sounding-board through a *ventroid*, as we find generally accepted upon the theory of Chladni. In remarks upon my paper at the Physical Society, Lord Rayleigh suggested that this matter could be demonstrated by cutting a tuning-fork out in both ends of a long steel bar in the manner I had done, for an experiment, in one end only; we might then observe if sonorous vibrations would be communicated through either of the prongs of the double fork, at the opposite end to that set in vibration. In the following week I constructed such a fork, and I found that either of the non-vibrating prongs, when the opposite ones were set in vibration, would form a perfect stem to the fork, and communicate sonorous vibrations just as well as a single stem. In this case it will be seen that the prongs, which may be considered to form the stem, lead directly to the places on the fork pointed out as its nodes by Chladni. It appears, therefore, evident that a node may communicate sonorous vibration to a sounding-board.

After reading my paper Dr. Stone told me in conversation that he had constructed a tuning-fork with a rod projecting at right angles to the open space between the prongs, and directly from its stem, and that this rod communicated sonorous vibrations from the fork to a sounding-board nearly as perfectly as the direct stem. This modified form of fork I also made by screwing a stem into my experimental fork, which was made in the end of a flat steel bar. I found it to act as Dr. Stone had stated. These experiments appear further to show that sonorous vibrations are communicated through nodes to sounding-boards. If we may apply this principle to stringed instruments, we must look rather to the bridge than the transverse motion of the string, as the communicator of the sonorous vibrations which produces the note. I may say that the discussion of Chladni's theory was not the object of my paper, the purpose of which was to show that the sonorous vibrations forming a note are possibly compounded of vibrations of much smaller amplitude than generally assumed, which was perhaps better demonstrated by other experiments.

W. F. STANLEY

The Mount Pisgah (U.S.) Stone Carvings

THE number of NATURE dated June 15 (p. 160) contains some statements relating to the curious stone carvings discovered by Mr. M. S. Valentine in the neighbourhood of Mount Pisgah, North Carolina, and now exhibited by him in Europe. Before leaving the United States, Mr. Valentine brought his specimens to Washington, in order to have them examined by Prof. Baird, the Director of the United States National Museum, and by myself. I am therefore enabled to express an opinion concerning them. Having been for many years in charge of the largest

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existing collection of North American antiquities, I can safely assert that they are totally abnormal in character, that is, unlike any pre-Columbian stone carvings thus far found in the United States. They neither show the characteristics of the stone sculptures discovered in mounds, nor do they resemble the well-known specimens of modern Indian art. In short, they are not typical at all, unless, indeed, we deem them sufficiently important to form a type for themselves. Such an importance, however, I cannot concede to them, believing that they originated in comparatively modern, certainly in post-Columbian, times, and were made by a few individuals of the Indian, or, perhaps, even of the Caucasian, race. The rude attempts at imitating animals of the Old World are conclusive evidences that the makers either had seen such animals, or knew at least that they existed.

The carvings, it should further be taken into account, are executed in soft potstone, a material easily yielding to the effects of exposure, and hence a short lapse of time would have sufficed to give them the appearance of real antiquities. In fine, I consider these carvings as a modern intrusion, and would deem it an extremely hazardous attempt to make them the basis for speculations bearing on the ethnology of North America.

CHARLES RAU,
Curator Department of Antiquities,
U.S. National Museum
Smithsonian Institution, Washington, D.C., June 27

The Influence of Light on the Development of Bacteria

IN NATURE for July 12, 1877, there appeared a short communication from Messrs. Downes and Blunt summarising the conclusions at which they had arrived as the result of investigations on the influence of light on the development of lower organisms. The experiments were described in detail in the *Proceedings* of the Royal Society for 1877 (vol. xxvi. p. 488), and were considered by them to show that light is inimical to the development of bacteria in Pasteur's solution; but that for the full effect direct insolation is needed. Exposure to the sun's rays, according to them, may simply retard development, or it may completely sterilise the solution, by killing bacteria or their germs contained in it. In a second paper read before the Royal Society (*Proceedings*, vol. xxviii. p. 199), some further experiments were detailed, which, however, did not, I venture to think, do much towards settling the difficulties of the question. In the same volume of the *Proceedings* (p. 212) Prof. Tyndall supplied observations of his own, which confirmed the conclusions of Messrs. Downes and Blunt, in so far as the retardation of development was concerned, but differed on the point of sterilisation by bacterial destruction being attainable by insolation. At the last meeting of the British Association, Prof. Tyndall returned to the subject (NATURE, September 15, 1881), and related some further experiments confirming what he had previously stated.

I have recently made a considerable series of experiments, with the hope that under our bright Australian sun I might get results decisive on the point of difference between these inquirers and also confirming or negativing the result on which they were agreed. I made use of Cohn's solution as the cultivation fluid and common one-ounce phials as the vessels. The solution was inoculated with a small quantity of fluid swarming with bacteria (*B. termo*), the bottles plugged with cotton wadding and exposed fully to the sun. Some of the experiments were made in the hottest weather of February and March, and the later in April. To begin with, I simply placed the bottles on the outside of a window-sill, on which the sun shone during the greater part of the day, a temperature of 124° F. being noted on one occasion, and that probably not the highest reached. My first results seemed fully to confirm the conclusions of Messrs. Downes and Blunt, complete sterilisation apparently being sometimes attained, at least as far as bacterial growth was concerned, the destruction of mould spores, as also noted by them, not being so easily accomplished. Suspecting at last that the effect might possibly be due as much to elevation of temperature as to any special chemical or other action of the sun's rays, I varied my procedure. I was led to do this in part by the circumstance that I had not seen diffused light check in any way the bacterial growth, when the solution exposed to it was kept at the same temperature approximately as that in other bottles closely wrapped in brown paper. It did happen, indeed, that the exposed solution became opalescent *sooner* than that which had been guarded. The method ultimately adopted was, to suspend the bottle outside of a window, and the particulars of one experiment will make clear what resulted, and show the general method. On April 6, at 2 p.m., the weather being bright but cool, these bottles, containing each two drachms of inoculated solution, were suspended outside of a window. The 7th was cloudy, the 8th bright and cool; and on the 9th, which was bright and warm, all were still found transparent, and, at 9 a.m., one was brought in out of the sun. On the 10th, which was also bright, another was taken in at 9 a.m., the one which was left out showing, at that time, faint signs of cloudiness. A thermometer hung up beside it marked a temperature of 98° F. Next day, the 11th, at 9 a.m., the exposed solution was quite milky, the others just beginning to show traces of opalescence, the one removed on the 9th being least advanced. This experiment, even by itself, was almost decisive. It established the fact that insolation by itself does not prevent the growth of bacteria in a perfectly transparent medium, and does not even retard it, relatively to the time needed in solution less exposed, but kept at a rather lower temperature. In another experiment I found two bottles continuously exposed to the sun during two bright days, become milky, the bacterial growth, in fact, only beginning then, no trace of cloudiness having shown itself during five previous days which were dark and cold.

The conclusion I came to of necessity was, that the bacterial development was mainly, if not wholly, dependent on temperature. On referring to a paper by Dr. Ed. Eidam, in Cohn's *Beiträge zur Biologie der Pflanzen*, Heft iii., I found that he had proved that the *bacterium termo* passes into a torpid condition (Wärmestarre), when exposed to a temperature of between 40° and 45° C. (104°–113° F.); is killed by seven days exposure at 45° C., by fourteen hours at 47° C. (116°·3 F.), by three to four hours at 50°–52° C. (122°–125·6° F.), and by one hour at 60° C. (140° F.). In this country there need be no difficulty in getting a heat in the sun, greater than even the highest of these; and I should think it possible enough in England, on a hot summer day, to get a temperature in exposed situations considerably over 104° F., sufficient to paralyse the bacteria, or even to kill them, if the exposure was long enough continued. Any explanation of the difference between the results of Messrs. Downes and Blunt, and those of Prof. Tyndall, on the point of complete sterilisation, is simply that, while they used small test-tubes, his solutions were contained in flasks, and that the larger body of fluid less easily reached the highest temperature attainable under the conditions. The different results, with very small and larger tubes, observed by the former, and to them evidently inexplicable, if not simply accidental, is best explained on the same principle.

It is true that Prof. Tyndall agrees with the other inquirers in disclaiming the notion that the apparently inimical influence of light can be ascribed to difference of temperature; but it is evident that it had not occurred to them, that any possible elevation of temperature could act otherwise than by favouring bacterial growth and multiplication. This is plain, from the words used by Messrs. Downes and Blunt (*Proceedings*, vol. xxvi. p. 491), when, trying to account for some anomaly, they remark that "external conditions—notably temperature—may retard or counteract the preservative quality of the solar rays. It must be understood, however, that the putrefactive tendency of warmth does not, in our experience, with this solution at least, override what we termed the preservative quality of light; for, provided that there was the full amount of sunlight, we have preserved tubes exposed *continuously* from day to day as readily in hot weather as in cool." Prof. Tyndall, in his recent paper, speaks of his flasks having been exposed to strong sunshine for a whole summer's day; and, with reference to the more rapid occurrence of turbidity in those which had been shaded, adds: "This result is not due to mere difference of temperature between the infusions. On many occasions the temperature of the exposed flasks was far more favourable to the development of life than that of the shaded ones."

I feel the boldness of criticising the conclusions aimed at by such a famous investigator as Tyndall, and all the more when these are in accord, in the main, with those of other inquirers, the joint results having hitherto, to all appearance, been accepted as unimpeachable. I do so only after careful observation and consideration, and with the hope that further investigations, made with due precautions, will establish the correctness of what I have here stated. My researches in detail will be brought before the Royal Society of Victoria at the next meeting, and I will take the liberty of forwarding you a copy of the paper when

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